THURSDAY, APRIL 20, 1882

ECLIPSE NOTES

IN the following notes I propose to discuss certain points which in my opinion it is desirable to investigate as fully and as carefully as may be during the coming eclipse.

The magnificent volume which astronomers have received from America during the last year, in which are garnered all the rich results, or most of them at all events, collected during the eclipse of 1878, may really be said to have brought to a focus the chief points of study which are open to us during eclipses. I shall, therefore, use this volume freely in connection with the various branches of research. But still there are points of interest which lie outside this book, for, since the year 1878, I for one, at all events, have been driven to the conclusion that our then views of the chemical and physical constitution of the solar atmosphere require considerable modification to make them accord with the facts.

I have taken many opportunities of showing that the various phenomena observed on the uneclipsed sun are more easily explained if we assume our chemical elements to be dissociated by the transcendental temperature of the sun, than if we hold that their molecular construction is the same there as here.

This question is one, the settlement of which is so important if it can be settled, that if an eclipse of the sun furnishes us with tests, it is our clear duty not to neglect them. I believe that an eclipse does furnish us with two or three such tests, and with reference to one of them, as I wish in these notes to bring together the various statements on the subject which have been made, I will begin by quoting from a discourse delivered by myself to the Astronomical Society last May. (Revised from a report in the *Observatory*.)

"The chemical constitution of the heavenly bodies is a question which necessitates some amount of attention from astronomers. Twenty years ago the observations of Kirchhoff and Stokes enabled us to get glimpses into the chemical constituents of the Sun. Nine years since, though we were in full presence of elements with which we are acquainted, other facts had been registered which exercised the minds of some observers. Kirchhoff's view was that the substances with which we are acquainted were demonstrated in the atmosphere of the Sun by an exact matching, both as regards wave-lengths and intensity, with the lines of certain chemical elements which he employed. Fraunhofer had earlier noted the coincidence of the bright yellow line of sodium with the line D. But Kirchhoff showed that not only in the case of sodium, but in iron, magnesium, and cobalt, and several other substances, there were coincidences which went to show that what was good for sodium was good for other bodies. But nine years ago we had not merely the opportunity of comparing these bright lines with the spectrum of the Sun's atmosphere as revealed by Fraunhofer, but we had the opportunity of studying the spectra obtained by observing very small portions of the solar atmosphere in regions where we should expect an exceedingly high temperature, namely, the inner regions of the solar atmosphere—the regions of spots and the regions of prominences. When we began to tabulate the lines thickened, the thing began to be very much less clear: of the 460 iron lines recorded by Kirchhoff only three were observed in the prominences. Then, when we got indication of a change of refrangibility of the lines due to the motion of the solar gases, we found about the year 1869 that the thickened lines which indicated iron vapour in the spots were not brightened in the prominences, so that a great many questions were raised; and when those questions were raised the idea of decomposition at a high temperature seemed to arise also. I bring before you to-night the results of some purely astronomical inquiries lately undertaken by the Solar Physics Committee. Of course a great many physical inquiries have necessarily entered into the researches. But the astronomical inquiries have had this object in view, namely—given the fact that a high temperature can decompose an elementary body, what happens to the spectra of those bodies when we examine the Fraunhofer spectrum, the spectrum of spots, and the spectrum of prominences? We have had before us the admirable work of Professor Young in 1872, but the work only lasted a month We felt we wanted more facts; so what we have been doing at Kensington during the last two-and-a-half years has been to obtain the spectra of 100 sun-spots—not a perfect record of all lines thickened, but results we could compare with Tacchini's; because, for prominences, we had to depend on Tacchini's observations, observations confined to the brightest lines of the prominences. The Committee therefore attempted something which was quite modest, and contented themselves with observing only the twelve lines most affected in Sun-spots. The question was, where to take the lines; and it was obviously the wisest course to take them in the most visible part of the spectrum; so that for two-and-a-hal. years we have been taking the twelve most widened lines between F and D. I will only trouble the Society with one set of these observations. At the top of this diagram[1] we have carefully chosen among the Fraunhofer lines, the lines stated by Angström to be coincident with the bright lines of iron; and we have given these lines of different lengths, the length representing the darkness of the Fraunhofer lines. In the next horizon we have the actual observations of the iron lines given by Angström, who used an electric arc with thirty or more of Bunsen's cells. We compared the intensities, also represented by length, as given by Angström and as given in the sun. You will see a considerable disparity. Below, we have the lines of Thalén, who used a powerful induction-coil, and the lengths of these also represent intensities. Comparing the Fraunhofer lines and Thalén's lines, you will see a still further disparity between the two spectra. Below, in these 100 horizontal strata are all the observations of the spots taken during the last two years. The first point which strikes one is the enormous number of iron lines, both in the solar spectrum and in the iron spectrum, which are not affected in spots or storms. It is as if on a piano only a few notes are played over and over again, always producing a different tune. The next point is the inversion of the phenomenon. If you examine the lines, you see that every line has been seen without the others. That hard fact is one which really is very difficult to understand, and what strikes one is the marvellous individuality, so to speak, of each of these lines. They do not go in battalions, or companies, or corporal's guards, but in single unities. The great importance of getting these observations was not so much for the observations themselves as for the comparison it enabled us to make with other observations; and naturally the next thing to do was to get a long series of observations of the prominences, because the prominences are hotter than the spots. The spots are caused by down-currents when the Solar atmosphere brings vapour from the cooler regions. They are opposed to prominences, which are ejections from the heated interior of the Sun. We have arranged here the observations of the prominences by Tacchini, since 1872. Here we are dealing with one substance—iron—over a very limited portion of the Solar spectrum; and what is the result? First of all you will see a very much greater

[1] See vol. xxiv. p. 322, Fig. 35.

simplification. The hottest part of the Sun has given us the fewest lines. Next, there is not a single line in common. Passing then from the iron lines in the spots to the iron lines in the storms, we pass from one spectrum

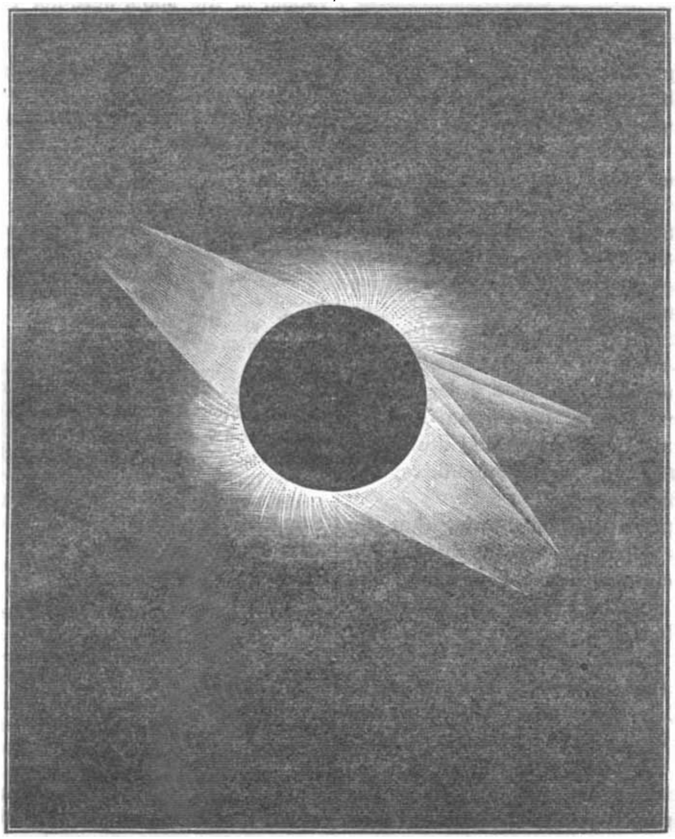

FIG. 1.—The Eclipsed Sun in 1878, from the photographs.

to another, and the two spectra are as distinct from one another as the spectrum of magnesium from that of chlorine or any other substance you please. I have ventured to put in red ink two other lines, because Tacchini

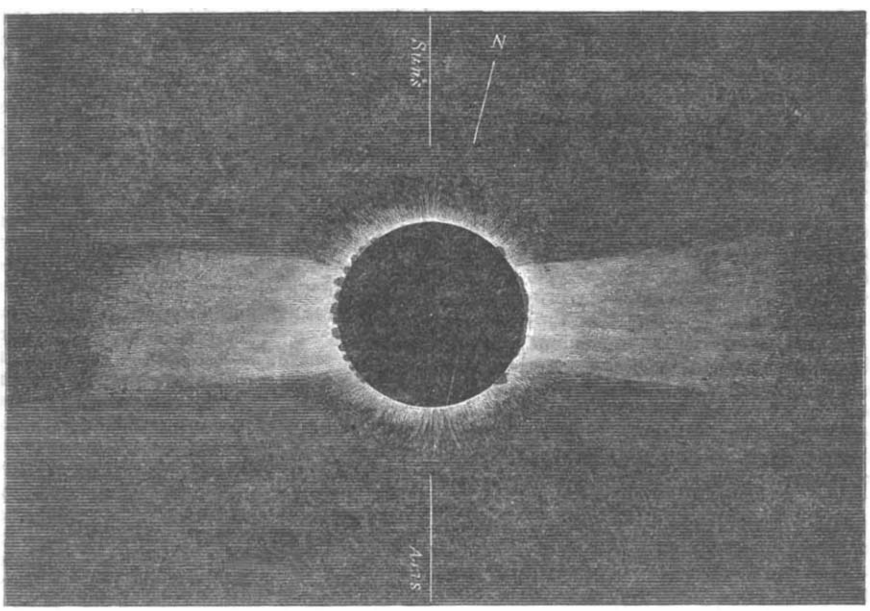

FIG. 2.—The Eclipsed Sun, August 29, 1867, as observed by Grosch at Colchagua, near Santiago.

found that about January 1873 the spectrum suddenly changed when the Sun was absolutely quiescent. There was no Solar rain, and we got the minimum of interference with local temperature. The iron lines van-

ished, and we got two new lines continued through a very long series of observations without any iron line at all; and these two lines have no Fraunhofer lines corresponding with them, nor do they appear in the spectrum of any chemical substance.[1] These phenomena are the last which one would expect. We can understand that differences in the quantity of the iron vapour present would make a certain difference in the spectrum; but we are driven to something quite independent of any change in the quantity of the iron vapour present. What, then, are we driven to? We see with every increase of temperature, passing from the general absorption of the sun to the absorption of the spots and to the radiation in the flames, increased simplicity, just as if a chemist were to talk to us about the action of temperature on substances which he has under control, and say the function of temperature was to simplify. Why, then, if this is the result of the working of temperature, why should not this simplification be due to the breaking up of the iron, if such iron exists at the exterior of the sun's atmosphere, into its finer constituents, as by the solar currents this iron is carried down into more highly heated solar regions? It has been stated there is no necessity for any view of this kind, but that the molecules of iron give out these vibrations, just as a series of bells vibrate differently according as they are struck in different ways. Fortunately, however, we need not have remained so long in doubt on a matter of this kind, because, as early as 1869 observations were made which showed that when the sun is in an excited condition iron vapours are among those vapours which show their motion by a change of refrangibility. So that we had the opportunity of learning whether these really were identical bells, so to speak, being struck in different ways. I think you will acknowledge

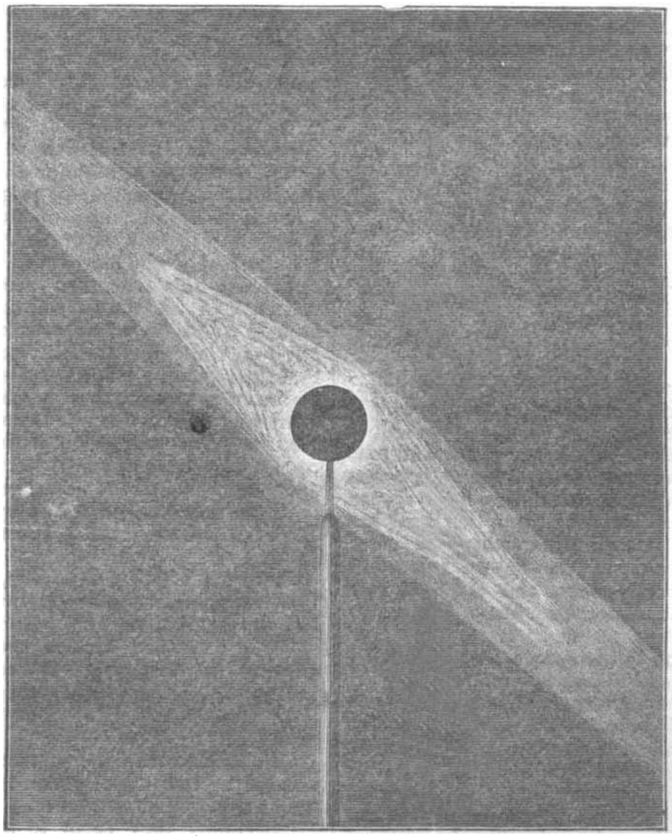

FIG. 3.—Prof Newcomb's observation (pp. 103, 104)

that if we are dealing with bells struck in different ways, however much the spectrum may vary, the molecules should be going with the same velocity. We found, however, when we came to make these observations, that the bells were going with different velocities; so that it cannot, by any possibility, be the same bells which on being struck give us those various notes. In another part of the spectrum these motions have been observed with very much greater success, for the reason that in that other part there are more lines which are observed to indicate considerable motion in Sun-spots. Limiting our observations to lines visible in the same field of view and at the same moment of time, it is a mere toss-up which line of iron shows a descending motion of thirty miles a second, and which line of iron does not move at all, either up or down; so that I think we are justified, so far as these observations go, in considering that there is great probability in favour of the view that we have in these lines, seen in spots and storms, the lines due to the constituents of iron, and not to iron itself, which are competent to resist the transcendental dissociating energies of these hotter parts of the Solar atmosphere. If so, we can bring it to the test; for if we accept any theory of evolution at all, we must imagine that, as our own Earth has cooled down, the Sun is cooling down; and if chemical forms are produced by that cooling, the complexity must be increased by reduction of temperature. If that be so, every reduction of temperature will be accompanied by increasing complexity of chemical forms, and then the highest temperature will be that condition in which we shall have the smallest number of elementary groupings of early forms. Dr. Huggins's work on the stars entirely justifies that view; and I want to point out the kind of test to

[1] See vol. xxiv. p. 368, Fig. 39.

which I allude. If these early forms really exist at the present moment in the hottest portions of the Sun, the spectrum of which is marvellously like that of Sirius, we ought not to expect these early forms to be confined to one of our earthly constituents. But what are the facts? The facts are very precise indeed. On this map we have the result of all the individual observations of the spots and the flames to which I refer. What we find is, that to every prominent line in the spots and in the storms, although these two have no line in common, there is a line common with our present instrumental appliances to iron, vanadium, and chromium, another common to iron and titanium and so forth; and the lines shown by Angström and Thalén, as common to two or more elements, are precisely those lines which are thickened in spots or are brightened in storms, so that the view we have here of early forms of matter is absolutely justified by this massing of lines here and there. We have been able to increase the number of 'basic' lines over this region by observing the lines constantly thickened in the spots. This does away at once with the idea that all these basic lines arise from the fact of the lines being double. For if they are to be doubled there is no reason why the 60 per cent. of lines neglected by the spots and the storms should not have been double lines. But neither Angström, nor Thalén, nor myself have picked up one of these basic lines when we passed from the atmosphere of the spots or the special atmosphere of the flames. Now, there is a moral to this, if you will allow me to enforce it. There is an eclipse of the Sun next year, lasting only, I am sorry to say, a minute and a very few seconds; but there is to be another the year after, lasting nearly six minutes, but it happens to be in a part of the world where it is always afternoon. In the observations of the future we must pay attention to these lines which have been picked out by nature herself in these spots and prominences. If I observed either of these eclipses, I should be content to fix my instrument on three iron lines between 4900 and 5000 ten millionths of a millimetre, because, of these three lines which are in the Fraunhofer spectrum, two have always been seen in spots without the third, and the third has always been seen in the prominences without the other two. If, then, the spectrum of the flames represents the lowest part of the atmosphere, and the spectrum of the spots represents the atmosphere above the flames and below the corona, than we ought to see these lines different in the corona, and in the corona we ought to see the lines which are dropped in these two regions. Of the twelve lines between 4,900 and 4,957, only one is picked out by Thalén for intensification, and that particular line is the line seen alone in the region of the prominences. There are eleven lines which are absolutely untouched by Thalén, showing, that absorption must be proceeding somewhere; and it is most interesting to determine where it is going on. In the Indian eclipse, in 1871, I saw these lines reversed before totality. I saw as it were hundreds of lines; but if I had confined my attention to the three lines I should have got a better idea of what the magnificent flashing out of those lines meant. It has been called the reversing layer; but I do not now believe it is the reversing layer for a moment; for, when it comes to be examined, we shall probably find that scarcely any of the Fraunhofer lines owe their origin to it, and we shall have a spectrum which is not a counterpart of the Solar spectrum."

As further thought led me to believe that this method of observation was one of the most important that could be employed next May, I laid great stress upon it in a memorandum which I subsequently submitted to the Government Committee on Solar Physics, and I pointed out to them at the same time that from what Captain Maclear and myself observed in India in 1871, there was a great probability, that on this question facts might be collected, not only at the exact moments of disappearance and reappearance of the sun, but perhaps even for two or three minutes both before and after totality, by keeping the slit of the spectroscope very carefully on a point where the cusps were narrowest.

The memorandum to which I have referred runs as follows:—

"The total eclipse of the sun which takes place in May next year will be visible in such an accessible region, that it is to be hoped that the precedents of 1860, 1870, 1871, and 1875 will be followed, and steps taken to secure observations, the more especially as the eclipse will happen somewhat near to the period of maximum sun-spots, and will allow of a comparison being made with the results obtained in India in 1871.

"There is one new point (it is not necessary now to refer to the importance of registering the ordinary phenomena) to which I beg to invite the attention of the Committee.

"The discussion of the sun-spot spectra recently observed at Kensington, and of the prominence spectra observed at Palermo by Tacchini, since 1872, throws some doubt upon the validity of some of the conclusions based upon the results obtained by the English and American Government Eclipse Expeditions in 1870.

"In that year, at the moment of the disappearance of the sun, a large number of bright lines was seen to flash out, and it was supposed that these lines composed the spectrum of a thin layer near the sun, and were those the reversal of which produced the lines of Fraunhofer.

"Hence this layer has been termed, and generally accepted to be, the reversing layer. The conclusion seemed to be in harmony with the results obtained by Dr. Frankland and myself, who gave reasons for showing that the region in which the absorption of the elementary bodies of greater atomic weight than hydrogen, magnesium, and sodium must be below the chromosphere. This view was put forward at a time when the elementary nature of the so-called elements was never questioned, and before any of the recent results had been obtained.

"The observations made by the Government Eclipse Expedition which went to India in 1871, showed that this flashing out of lines was a real phenomenon; but as the observation was a general one, and as during the eclipse the Fraunhoferic lines were invisible, there was no absolute demonstration of the identity of the two spectra.

"The facts, now beyond question, that *quâ* the same element, the spectra of spots and flames differ, and that the spectra differ widely among themselves, throw great doubt upon the conclusion to which reference has been made.

"First they seem to indicate that some of the absorption takes place at a higher level than that occupied by the so-called reversing layer.

"Secondly they seem to indicate that many of the brightest lines seen during the flash to which reference has been made may be those seen thickened in spots or intensified in the prominences, although they do not occur except as excessively faint lines among the Fraunhoferic lines.

"In short, in 1870, the fact that the spot and prominence spectra are so widely different from the ordinary solar spectrum, had not received the attention it must receive in the light of the most recent inquiries, and it was taken for granted that because a large number of lines was seen, that therefore they occupied the same positions as the large number of lines which compose the ordinary solar spectrum.

"The recent work seems to show that the complete absorption spectrum of any one element is produced, not at one level, but at various levels, the absorption of all the levels being added together to give us the complete result.

"If this be so, the lines seen in the flash will not all be Fraunhoferic lines with the ordinary intensities.

"A crucial test, which can only be applied during an eclipse, and with difficulty then, will be to observe what happens during the flash to those lines which are specially picked out for intensification in spots and flames. We might expect to see the lines untouched in spots, the lines thickened in spots, the lines brightened in prominences, stretching to different heights.

"They would all appear to rest on the moon's limb, or on the sun's limb, if the cusps can be observed, because we are dealing with the section of a spherical mass, or rather, perhaps, of zones of concentric spherical strata.

"To apply this test under the best conditions, adjacent lines should be taken with cross wires, or some equivalent arrangement adjusted on the corresponding Fraunhoferic lines before totality.

"The iron lines at 4918·0, 4919·8, and 4923·1 will be the best to observe for this purpose, as they are close together, and two are always absent from prominences, and one is never thickened in spots."

When it was decided that an attempt should be made to secure observations of the coming eclipse, the next thing to do was to try to get over the tremendous difficulty that we have always experienced, namely, that during the eclipse itself, the sun's light, and therefore its spectrum, were absent, so that our familiar scale of reference is lost. This is at last got over in a manner so simple that the only wonder about it is that it has not been thought of before. I allude to the employment of a photograph of that part of the solar spectrum which we want, instead of micrometer wires or any other more elaborate means of determining positions, and this method I have already tested, and it works remarkably well.

What is requisite is that instead of a camera replacing the eyepiece it should really form part of it. The plate can be taken away and the eyepiece may be used in the ordinary manner, or a sensitive plate may be placed in it, and a photograph taken. It may then be taken out and developed, half of it wiped off before it is exactly replaced in its original position, and then we have a field of view, the eyepiece never having been separated from the camera during the whole of this time, half of which is occupied by the photograph, the other half with the spectrum of that part of the solar atmosphere which it is desired to study.

The instruments to be used during the eclipse—both telescope and spectroscope—will be identically those with which Capt. Maclear and myself observed the bright lines in 1871, so that instrumentally the chances are good.

I have already pointed out that it is necessary that the slit should lie on the narrowest point of the cusps. To secure this a 3¾″ finder of exquisite definition has been solidly fitted to the telespectroscope with adjustments easy of application which shall insure this result, and in order that the observations may be continuous both in the presence and in the absence of the sun, a diagonal eye-piece with a prism twice the usual size, is employed. This slides easily in two grooves. Half of it is silvered, half of it not, and at the instant of totality the silvered portion is thrown into use.

It is hardly necessary to add that the slit of the spectroscope can be made to lie at any angle from the normal.

So much, then, for one possible test of the new views. There is another—not perhaps quite so direct, but one which it will be still of interest to make. Since 1871, when Janssen made the first observation of this nature, those observers who have studied the spectrum of the corona under good conditions with small dispersion have seen some dark lines as well as the ordinary bright ones, and it has been assumed for the most part that these dark lines are simply the dark lines of the ordinary sunlight reflected to us by particles in the solar atmosphere.

The possibility of putting this question at rest in the most absolute manner by comparing the spectrum of the corona with a photograph of the green part—that is to say, the most luminous part of the solar spectrum (for too much light must not be expected), renders this observation one of importance to make, and, thanks to Capt. Abney's recent researches in the science of photography, it is now as easy, however confusing it may be to those who believe in chemical rays, to obtain a photograph of the green as of the blue, and this will be done before the eclipse.

There is reason to think that if the new views have any truth in them the spectrum of the corona may—I do not say must—give us the ordinary solar lines changed considerably in intensity, but it is probable that this observation will be a delicate one at the best.

But more than our views have changed since 1878. The photographic attack now requires seconds only where formerly minutes were wanted. Nor is this all: the red end of the spectrum awaits a record which it is now easy to secure. Indeed, thanks to Capt. Abney's skill, plates have been prepared which it is hoped will grasp the red and green and blue light with equal vigour, so that one can now more than dream of a permanent record of the whole spectrum from the invisible violet at one end to the invisible red at the other.

We got the first photograph of the spectrum during an eclipse by means of instruments constructed in 1875 for the Siam eclipse in that year. In these instruments I employed a method first used by Fraunhofer, to save as much light as possible. The corona was its own slit and the prism was placed in front of the object-glass, and the dispersion of the prism used was small, because the method was new, the plates were slow, and we were anxious to secure something. We now know that we may safely go ahead, and a prism 3½ inches square in the side, of 60°, will be placed in front of a lens of 22 inches focus.

The length of the spectrum, if all goes well, will be four inches, including the infra-red, which Capt. Abney believes will be recorded in one minute's exposure, and this will be available in an eclipse of 72 seconds.

These extremely rapid plates enable other attempts to be made which formerly would have been considered hopeless. The ordinary photographs of the corona will be taken (by a lens of 5 feet focal length and 4½ inches in diameter) on plates sixty times more rapid than those prepared on the old process. This fact must be insisted upon, because it is evident that the shortness of the totality during the present eclipse is not such a drawback as it once would have been.

Another attack will be as follows:—An image of the sun will be thrown on the slit of a spectroscope by means of a heliostat and condensing lens. The size of the solar image thus obtained will be about ⅛ of an inch. The beam of light will be dispersed by a flint prism of 2

inches face, of 60°, and the spectrum will be brought to a focus on a sensitive plate by a lens of a mean focus of 9 inches. An attempt will be made to secure the whole of the spectrum, and for this purpose the plate requires to be inclined at an angle of 40° to the axis of the lens. The spectrum which it is hoped to obtain by this arrangement would have required an hour's exposure some years ago.

With this instrument attempts will be made to secure a photograph of the flash of bright lines at the beginning of totality, and the spectrum of the corona during totality, an arrangement being made for a comparison solar spectrum after totality by shutting off half of the slit.

So much, then, for the work suggested by applying views and methods which have been broached since the last eclipse.

The remarkable form of the corona, and its still more remarkable extension in 1878, and its great variation from that seen in prior eclipses with a single exception—that of 1867—will render observations of the form and extent of the corona in the present year of the highest importance, even if we had not Dr. Siemens' suggestive hypothesis to lend a more than usual interest to it.

I give an illustration (Fig. 1) copied from the American volume, which I owe to the courtesy of the Superintendent of the Naval Observatory at Washington, to show how my own observations of that eclipse, an account of which was sent to NATURE from America at the time, have been borne out by a discussion of the photographs. Side by side with it, in order that the equatorial extension and the almost identical tracery at the poles can be seen, I give a copy of a drawing made in 1867, both sets of observations having been made four years before the sun-pot maximum (Fig. 2).

Here, indeed, we have food for thought; for the currents in the solar atmosphere, revealed by these drawings, seem to be exactly those demanded by Dr. Siemens; and indeed, his hypothetical diagram which appeared in NATURE a few weeks ago, should be compared with them, in order that the points of resemblance may be grasped.

With reference to the other drawing (Fig. 3), which shows the remarkable observation made by Prof. Newcomb, I cannot do better than make the following quotation from the volume in question:—

"It had always seemed to me that the visual study of the faint outlying portions of the corona would necessarily be interfered with by the brilliant interior portions unless the view of the latter were intercepted. I therefore made preparations to repeat the experiment unsuccessfully attempted at Desmoines in 1869, of hiding the central corona by a screen about 1° in diameter, and examining such portions as might be visible outside of it. The screen now used was made of wood, about 12 inches in diameter, and was mounted on top of a telegraph pole which was set on the elevated ground to the west. The altitude and azimuth of the sun at the moment of central eclipse were carefully calculated, and the screen fixed in such a position that when viewed from the top of a stake driven in the ground alongside of my telescope it would cover the eclipsed sun. The angular diameter of the screen as measured with a sextant from the stake was 57′, its distance was about 60 feet. As this would cut off about 12′ of the corona all round the moon I considered it ample for the purpose, but the results showed that it might well have been somewhat larger.

"I remained in the dark room until about three minutes before the commencement of totality with the view of having my eyes as sensitive as possible. I then walked to the telescope, keeping my eyes partially protected from the light. The lurid colour of the landscape was very striking. The light seemed no longer to be that of the sun but rather to partake of the character of an artificial illumination. This appearance is very readily explicable by the fact that the light coming only from the limb of the sun belongs principally to the red end of the spectrum. As the last ray of sunlight was disappearing I stepped to a stake driven into the ground, the top of which marked the point from which the sun would be entirely hidden by the screen. A bright corona was plainly visible all round the screen, although a portion 12′ from the limb of the sun was entirely cut off. My attention was immediately attracted by a faint blush of light, extending out on each side at an angle of about 45° with the horizon, each end terminating in a long narrow ray. I made a very careful estimate of the length of these rays as 6° from the disc. They shaded off by insensible gradations, and struck me as having a great resemblance to a representation of the zodiacal light on a reduced scale. They were to all appearances continuous with the corona. With a view of judging whether their direction coincided with that of the ecliptic, I tried to judge whether the western one pointed towards the planet Venus, then plainly visible near the horizon. Its direction was apparently very slightly below that of the planet.

"The outlying portions of the corona other than those rays were extremely irregular; that is, there were several rays and other irregularities extending out in different directions. As these were common phenomena, I took no note of their details." J. NORMAN LOCKYER

April 18

(*To be continued.*)

PROF. WIESNER ON "THE POWER OF MOVEMENT IN PLANTS"

Das Bewegungsvermögen der Pflanzen: eine kritische Studie über das gleichnamige Werk, von Charles Darwin, nebst neuen Untersuchungen. Von Julius Wiesner. 8vo, pp. 212. Three Woodcuts. (Wien: Hölder, 1881.)

BEFORE attempting to reply to some of Prof. Wiesner's criticism, it is a pleasure to record my appreciation of the courteous spirit in which his book is written, and the uniformly respectful tone which he employs towards my father. His criticism is so extensive that there is hardly a single point of any importance in "The Power of Movement in Plants" with which Prof. Wiesner agrees. Yet in spite of this far-reaching difference of opinion, he is good enough to express himself warmly as to the value which the book possesses.

Wiesner devotes a good many pages to Circumnutation, and as this phenomenon and the theories connected with it form an important part of "The Power of Movement in Plants," I shall begin with this question. In the first place Wiesner finds fault with one of the methods employed by us in our observations on circumnutation, and gives a diagram (Fig. 3, p. 161) which shows that the method may lead to false conclusions. In the method of observation criticised by Wiesner, the position of the plant at any moment was determined by making a dot on a glass plate in such position that it was in a line with a mark on the organ whose movements were to be observed and with a stationary mark behind or below it. This method is obviously open to objections, and we never ima-